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SPORE DISTRIBUTION IN LIVERWORTS.—It has no doubt been noticed by all observers of the liverworts that, while terrestrial species have as a rule (*Riccia* and *Sphaerocarpus* are exceptions) their capsules raised on elongated stalks furnished either by sporophyte or gametophyte, those which grow on trees seldom elongate their stalks more than enough to free the capsule from the perianth. This difference is plainly due to the fact that the arboricolous species are sufficiently elevated to allow their spores to be well scattered without any special contrivance. It is interesting to note, however, the behavior of the fertile branches of *Porella*

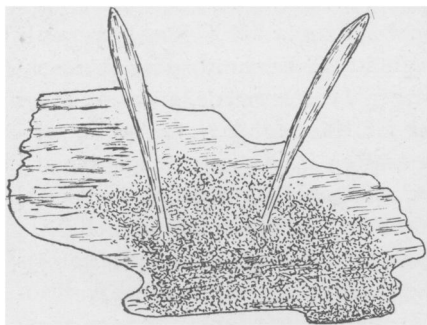


FIG. 16.—*Clavaria mucida*, growing on alga-covered wood. $\times 5$.

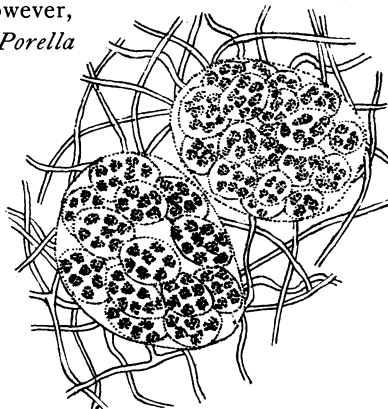


FIG. 17.—*Clavaria mucida*. Hyphae surrounding groups of algae. Highly magnified.

platyphylla Lindb. While the vegetative branches of the liverwort remain closely appressed to the bark of the tree, the fertile shoots bend away some time before the spores are ripe, and often project a centimeter or more from the substratum. This exposes the spores to the free play of the wind and no doubt prevents many of them from being caught by the leaves of the mother plant. This habit seems to show that even in arboreal forms it may be an advantage to have the capsule removed some distance from the substratum. It will be noticed here that *Porella* resembles the Marchantiaceae in giving over to the gametophyte the duty of lifting the capsules.—W. C. COKER, *University of North Carolina, Chapel Hill*.

ON THE INTERPRETATION OF THE QUADRIPOLEAR SPINDLE IN THE HEPATICAE.

SEVERAL papers have appeared recently bearing on the subject of the quadripolar spindle in the Hepaticae, particularly in connection with the genus *Pellia*. As the writers do not always seem to have

apprehended the views I put forward in 1895 respecting this structure, or the degree of importance I attached to it, I may perhaps be permitted to explain the position I then took up, especially as I have seen no reason seriously to depart from it since that time. The structure in question was first seen by me in *Pallavicinia decipiens*. In this form the deeply lobed character of the spore mother-cell is marked in the highest degree, and I afterwards found the same structure in other Jungermanniales in which the spore mother-cells present similar characters.

Now I expressly regarded the appearance of four centrospheres, whether I was able to distinguish centrosomes (Fossombronia) or not as bodies contained within them, as a phenomenon *to be correlated with the lobed character of the cell taken as a whole*. But I certainly did not regard its persistence throughout the first mitosis as an essential condition. It does not so persist in Fossombronia, or in any other forms but *Pallavicinia decipiens*, and far less regularly in Pellia.⁷ In summing up the body of evidence, I said (*l. c.*, p. 510): "The quadri-polar spindles of these Hepaticae are really only the result of the special conditions imposed by the configuration of the spore mother-cell."

It is clearly, therefore, a misleading rendering of my position, as adopted in 1895, to suggest that the quadrupling of the primary chromosomes and their simultaneous distribution into four groups to form the daughter-nuclei constitute the "most remarkable features of Farmer's account of the activities of the quadri-polar spindle."⁸

From the first, as soon as I began to extend my observations made on *Pallavicinia decipiens* to other species of Hepaticae, I recognized that this formed a very special case, and I correlated it with the unusually deep lobing of the spore mother-cell. I do not myself at all regard the quadrupling of the chromosomes and their simultaneous distribution as the point of central interest. It is even possible that future investigation, with the help of more modern methods, may show that the two divisions were not so much compressed as I thought in 1894, and that a cell plate may after all be formed during the first mitosis.

I may say in passing that I am not surprised that the character of the mitosis in *P. Lyellii*, described by A. C. Moore, should differ from that in *P. decipiens*. He depicts a slightly four-lobed cell, somewhat recalling that of Fossombronia, or Aneura, and I am much interested to see that, as one would have anticipated, the general features of the karyokinesis resemble those presented by the species of the latter genera with which I am familiar. A. C. Moore is right in supposing that I should interpret his *fig. 1* as indicating a less pronounced stage of

⁷ Annals of Botany 9:475.

⁸ MOORE, A. C., BOT. GAZ. 35: 388. 1903.

what in *P. decipiens* becomes a very well marked four-rayed figure. I observe he does not figure centrosomes or centrospheres, such as are easily seen at the corresponding stage in *Aneura* or *Fossombronina*. I fancy these bodies are not always identically developed, perhaps even in closely related forms. I have become convinced that in many liverworts there are often present within the centrosphere, where it can be distinguished, one or more particles that may be looked on as representing a centrosome. The number is, I think, of no great moment. The existence of the *substance*, whether in one or more granules, is the essential point. A nucleolus does not cease to be a nucleolus because several of them may be present in a nucleus which more frequently only contains a single one, and the same argument applies, I think, to the centrosome substance where it can be identified at all.

And as to the quadripolar spindle, whether we choose to restrict the term "spindle" to that portion of the achromatic apparatus that has become continuous from pole to pole, or whether we prefer to extend it so as to include the fibers or substances that are differentiated between the centrospheres and the nucleus during the prophase, irrespective as to whether these retain their positions later or not, is a matter of individual taste. Personally, seeing that they form a stage in the differentiation of the interpolar spindle (with or without fusion in pairs or otherwise), I embrace the latter alternative; and so include the four-rayed figure, independently of its degree of development or permanence as part of the spindle apparatus. I do not think much is gained by limiting the term "spindle" to the later stages, or restricting it purely to bipolar forms.

I should like to take this opportunity of correcting a statement in the memoir on nuclear division to which I have already referred in this note. At the time that paper was written the terms "heterotype" and "homotype" had not acquired that definite meaning that now attaches to them. At the present time I should certainly not regard the second division of the spore mother-cell as in any case in essential characters being really of a heterotype nature. The small size of the objects makes an exact appreciation of their evolution and final form a matter of some difficulty. I have, however, no reason to suppose that those few instances in which a renewed examination of the question has not resulted in definitely settling their homotype character, do really in any way differ in this respect from the many forms in which the nature of the second mitosis can be satisfactorily ascertained.—
J. B. FARMER, *London, England.*